3 Nomological machines and the laws they produce

1 Where do laws of nature come from?

Where do laws of nature come from? This will seem a queer question to a post-logical-positivist empiricist. Laws of nature are basic. Other things come from, happen on account of, them. I follow Rom Harré[1] in rejecting this story. It is capacities that are basic, and laws of nature obtain – to the extent that they do obtain – on account of the capacities; or more explicitly, on account of the repeated operation of a system of components with stable capacities in particularly fortunate circumstances. Sometimes the arrangement of the components and the setting are appropriate for a law to occur naturally, as in the planetary system; more often they are engineered by us, as in a laboratory experiment. But in any case, it takes what I call a *nomological machine* to get a law of nature.

Here, by *law of nature* I mean what has been generally meant by 'law' in the liberalised Humean empiricism of most post-logical-positivist philosophy of science: a law of nature is a necessary regular association between properties antecedently regarded as OK. The association maybe either 100 per cent – in which case the law is deterministic, or, as in quantum mechanics, only probabilistic. Empiricists differ about what properties they take to be OK; the usual favourites are sensible properties, measurable properties and occurrent properties. My objections do not depend on which choice is made. The starting point for my view is the observation that no matter how we choose our OK properties, the kinds of associations required are hard to come by, and the cases where we feel most secure about them tend to be just the cases where we understand the arrangement of capacities that gives rise to them. The point is that our knowledge about those capacities and how they operate in given circumstances is not itself a catalogue of modalised regularity claims. It follows as a corollary from my doctrine about where laws of nature come from that laws of nature (in this necessary regular association

[1] *Cf.* Harré 1993.

sense of 'law') hold only *ceteris paribus* – they hold only relative to the successful repeated operation of a nomological machine.

What is a nomological machine? It is a fixed (enough) arrangement of components, or factors, with stable (enough) capacities that in the right sort of stable (enough) environment will, with repeated operation, give rise to the kind of regular behaviour that we represent in our scientific laws. The next four chapters will argue for the role of nomological machines in generating a variety of different kinds of laws: the laws we test in physics, causal laws, results in economics and probabilistic laws. This chapter aims to provide a sense of what a nomological machine is and of why the principles we use to construct nomological machines or to explain their operation can not adequately be rendered as laws in the necessary regular association sense of 'law'.

2 An illustration from physics of a nomological machine[2]

Consider the naturally occurring regularity I mentioned above: planetary motion. Kepler noted that Mars follows an elliptical orbit with the sun at one focus. This is described in the first law that bears his name. Since the time of Robert Hooke and Isaac Newton, the so-called 'Kepler's problem' has been just how to account for such an observed regularity in terms of mechanical descriptions, that is, using descriptions referring to material bodies, their states of motion and the forces that could change them. Specifically the account calls for the mechanical description of the system in terms of an arrangement of two bodies and their connection. In my terminology the task is to figure out the nomological machine that is responsible for Kepler's laws – with the added assumption that the operation of the machine depends entirely on mechanical features and their capacities. This means that we have to establish the arrangement and capacities of mechanical elements and the right shielding conditions that keep the machine running properly so that it gives rise to the Kepler regularities.

The basic insight for how to do so was shared not only by Hooke and Newton but also by their successors well into our century, for instance, Richard Feynman.[3] If a stone attached to a string is whirling around in a circle, it takes a force to modify its direction of motion away from the straight path so as to keep it in the circle. What is required is a pull on the string. In abstract physical terms, it takes a radial attractive force – a force along the

[2] I want to thank Jordi Cat for discussions and for contributing significantly to this section of the chapter.

[3] Feynman 1992.

radius of the circular path and towards its centre. In the case of the orbiting planet, the constituents of the nomological machine are the sun, characterised as a point-mass of magnitude *M*, and the planet, a point-mass of magnitude *m*, orbiting at a distance *r* and connected to the former by a constant attractive force directed towards it. Newton's achievement was to establish the magnitude of the force required to keep a planet in an elliptical orbit:

$$F = -GmM/r^2$$

where *G* is the gravitational constant. The shielding condition is crucial here. As we well know, to ensure the elliptical orbit, the two bodies must interact in the absence of any additional massive body and of any other factors that can disturb the motion.

Newton solved Kepler's problem by showing that the elliptical geometry of the orbit determines the inverse-square kind of attraction involved in the gravitational pull.[4] Conversely, he also showed that an attraction of that kind in the circumstances described could give rise to the observed regularity of the elliptical motion of Mars.[5] Although his proofs were essentially geometrical in character, the well-known equivalent analytical proofs were soon introduced and adopted.[6] In both cases, built into the mechanical concept of *force* is the assumption that in the right circumstances a force has the capacity to change the state of motion of a massive body. To account for the regular motion of the different planets in the solar system, we must describe a different machine, with a modified arrangement of parts and different shielding conditions. The pull of the additional planets on any planet whose observed regular orbit is being considered is then added to the pull exerted by the sun

[4] See I. Newton, *Principia*, Proposition 11. The proof solves the so-called 'direct Kepler's problem'. See Newton 1729.

[5] *Ibid.*, Proposition 13. The proof provides a solution to the so-called 'inverse Kepler's problem'.

[6] A sketch of the analytical proof goes as follows. In the relation between a force, the mass of a body and the acceleration the body undergoes as a result of the force (alternatively, the acceleration by virtue of which it is able to exert the force – Newton's second law of motion – the force can be expressed as a function of the position of the body and of time: $\mathbf{F}(\mathbf{x},t) = m\, d^2\mathbf{x}/dt^2$. A transformation into polar co-ordinates, the radius r and the angle ϕ, allows for an expression of the force in terms of the radial co-ordinates, $F(r,t)$ and the angular ones, $F(\phi,t)$. By eliminating the time parameter, one can obtain an expression for the force in terms of r and ϕ only: $F(r,\phi) = -l^2/mr^2(d^2(1/r)/d\phi^2 + 1/r)$, where l is the angular momentum of the system. This is the 'polar orbital equation'. Then by differentiating the orbital equation of the ellipse, $1/r = c(1 + e\cos\phi)$ (e is the eccentricity and c is a constant), one can arrive at the inverse-square form of the required force, $F = -k'(1/r^2)$, in the direction of the sun, where k' is empirically determined by the arrangement of the system ($k' = GmM$). A discussion of Newton's geometrical proofs and their correspondence with the modern analytical substitutes can be found in Brackenridge 1995.

in the expression of the gravitational force. The resulting orbits typically deviate from Kepler's perfect ellipses.[7]

The example of the planetary motions is important for me since it has been used by philosophers and physicists alike in support of the view that holds more 'basic' regularities as first and fundamental in accounting for observed regularities (i.e., in explanation, it is laws 'all the way down'). This view emphasises the unifying power of the appeal to Newton's laws with respect to Kepler's. I do not deny the unifying power of the principles of physics. But I do deny that these principles can generally be reconstructed as regularity laws. If one wants to see their unifying power, they are far better rendered as claims about capacities, capacities that can be assembled and reassembled in different nomological machines, unending in their variety, to give rise to different laws. Newton's 'law of gravitation' is not a statement of a regular association between some occurrent properties – say masses, distances and motions. For it does not tell us about the motions of two masses separated by a distance r, but, instead, about the *force* between them. The term 'force' in the equation of gravity does not refer to yet another occurrent property like mass or distance that could appear in a typical philosopher's list of occurrent properties. Rather, it is an abstract term (in the sense of chapter 2) that describes the capacity of one body to move another towards it, a capacity that can be used in different settings to produce a variety of different kinds of motions.

Those who advocate laws as fundamental also point to the heuristic role they play in scientific discovery. Thus Feynman writes, '[W]hen a law is right it can be used to find another one. If we have confidence in a law [e.g., Newton's law of gravitation], then if something appears to be wrong it can suggest to us another phenomenon.'[8] Feynman is referring to the discovery of Neptune. The belief in Neptune's existence was suggested by the irregularity that the orbit of Uranus displayed with respect to the predictions that can be made from Newtonian principles. In the law-first view, this discovery speaks to the importance of universal laws. I think this claim is mistaken. The observed irregularity points instead to a failure of description of the specific circumstances that characterise the Newtonian planetary machine. The discovery of Neptune results from a revision of the shielding

[7] It is worth mentioning that Newton's nomological machine derives its unifying power from its ability to account in addition for the regularities described in Kepler's second and third law. But in the case of the third law (that the square of the period of a planet's motion is proportional to the cube of the major axis of its orbit) Newton's description of the setting includes the assumption that the planet's mass is negligible compared to the mass of the sun. Kepler's law describes then only an approximation to the actual regularities displayed by the larger planets, such as Jupiter and Saturn. *Cf.* Goldstein 1980, p. 101.

[8] Feynman 1992, p. 23.

conditions that are necessary to ensure the stability of the original Newtonian machine.[9]

3 Models as blueprints for nomological machines

My views about nomological machines come primarily from my work on models in the LSE Modelling and Measurement in Physics and Economics Project. When we attend to the workings of the mathematical sciences, like physics and economics, we find the important role models play in our accounts of what happens; and when we study these models carefully we find that they provide precisely the kind of information I identify in my characterisation of a nomological machine. Let us consider first models that lie entirely within a single exact science, such as physics or economics, where the role of the word 'exact' is to point to the demand made on models in these disciplines that the behaviour to be explained within the model should be rigorously derived from facts about the model plus the principles of the theory. I consider a number of different models in various chapters here. On the physics side these include Newton's planetary models for Kepler's laws, which I just discussed; the detailed model provided by the Stanford Gravity-Probe team for the predicted precession of the four gyroscopes they are sending into space, from chapter 4; and the BCS model for the regular behaviour described in London's equations, from chapter 8. There are in addition two extended examples from economics: one in chapter 6 describes the regularities about efficiencies and inefficiencies that arise when debt contracts are written in certain ways and the other, an association between the length of time that individuals are unemployed and the persistence of unemployment in the economy, from chapter 7. All of these models provide us with a set of components and their arrangement. The theory tells us how the capacities are exercised together

In order to do this job, the capacities deployed in the models we construct in the exact sciences will differ from the more ordinary capacities we refer to in everyday life. Consider, for example, Coulomb's law. Coulomb's law describes a capacity that a body has *qua* charged. It differs from many everyday ascriptions of capacities in at least three ways that are important to the kind of understanding that exact science can provide of how a nomological machine operates. First, the capacity is associated with a specific feature –

[9] Of course, the alleged universality of the capacity of two masses to attract one another as described in Newton's principles does matter for that is the usual justification for the assumption that *these* particular masses will attract each other. Weaker assumptions about the extent of the capacity claim would clearly serve as well if the assumption of true universality should seem too grand.

charge – which can be ascribed to a body for a variety of reasons independent of its display of the capacity described in the related law – here Coulomb's law. This is part of what constitutes having a scientific understanding of the capacity. Contrast two more everyday ascriptions. I am *irritable* and my husband is *inaccurate*. These are undoubtedly capacities we have. Ask the children or anyone we work with. Each has been established on a hundred different occasions in a hundred different ways. Like the Coulomb capacity, these too are highly generic. They give rise to a great variety of different kinds of behaviour; the best description of what they share in common is that they are displays of my irritability or Stuart's inaccuracy.

These everyday cases contrast with the scientific examples that I am concerned with in the ways we have available to judge when the capacity obtains and when it does not. No one claims in cases like irritability to point to features which you could identify in some other way, independent of my displays of irritability, that would allow you to determine that I am indeed irritable. Philosophers debate about whether there must be any such features: first, whether there need be any at all in the individual who has the capacity, and second how systematic must be the association between the features and the capacity across individuals. Whatever the answer to these questions about everyday capacities, part of the job in science is to find what systematic connections there are and to devise a teachable method for representing them.

The second way that Coulomb's capacity differs from everyday ones is that it has an exact functional form and a precise strength, which are recorded in its own special law. Third, we know some very explicit rules for how the Coulomb capacity will combine with others described by different force laws to affect the motions of charged particles. What happens when a number of different forces are exerted together on the same object? To find out, we are taught to calculate a 'total' force ($\mathbf{F}_t$) by vector addition of the strengths and directions recorded in each of the related force laws separately. Then we use the formula $\mathbf{F}_t = m\boldsymbol{a}$ to compute the resulting acceleration.

These two features are characteristic of the study of capacities in exact science, although the method of representation varies significantly across domains, both for the capacities themselves and for how to calculate what happens when they operate jointly. For an example of a different method of representation in physics we can move from the study of bodies in motion to that of electric circuits. The capacities of the components of a circuit – resistors, capacitors, inductances, and impedances – are represented in well-known formulae. For instance, the *capacitance* of an isolated conductor is $C = Q/V$. How do we calculate the current in a complex circuit from knowledge of the capacities of its components? We reduce the complex circuit to a simpler equivalent one that has elementary well-known behaviour, using some selection from a vast variety of circuit-reduction theorems, such as

Thévenin's theorem or Millman's theorem. Then we use what is essentially Ohm's law ($I = V/R$) to calculate the current.

In game theory various concepts of equilibrium describe what is supposed to happen when the capacities of different agents are all deployed at once. Since we will look in some detail at an example from game theory in chapter 6, here let us turn to the more descriptive side of economics, to econometrics. In econometrics strengths of capacities are generally represented by coefficients of elasticity, which can often be measured by partial conditional expectations. There are two different standard ways for calculating what happens under combination. One is similar to vector addition. We add together the canonical influences from each feature separately. So we end up with a linear equation:[10] effects are represented by the independent variable, different causes by the dependent variables, where the respective coefficients represent the 'strength' of the capacity of each separate cause.[11]

In different situations we proceed differently: we use a set of simultaneous equations to fix what happens when different capacities are exercised together. Each separate capacity is represented by a different equation. When a number of capacities are exercised together, all the equations must be satisfied at once. Consider the simple case of supply and demand. The capacity of price to affect quantity supplied is represented in an upward sloping line; its capacity to affect quantity demanded by a line that slopes downward.

$$q_s = \alpha p + \mu \qquad \alpha > 0$$

$$q_d = \beta p + \nu \qquad \beta < 0$$

If the system is in equilibrium, the quantity supplied equals the quantity demanded:

$$q_s = q_d$$

What happens when both capacities of the price are exercised together? We require then that all the equations be satisfied at once. This means that the price is fixed; it lies at the intersection of the supply and demand curves. This is the source of the well-known identification problem in economics: how do we identify the equations we should use to represent the supply and demand equations separately when the supply mechanism and the demand mechanism never work on their own? What happens is always far more limited than what either equation allows, since the patterns of behaviour permitted by one are always further constrained by the second. The problem is

[10] It is of course possible to introduce more complicated functional forms, as we generally do in physics. One cost is that most of the statistical techniques we usually employ for testing will no longer be available.

[11] These kinds of cases were considered at length in Cartwright 1989.

especially pressing here because it does not even make sense to think of either of the two capacities being exercised on its own.[12]

These examples bring out the wholistic nature of the project we undertake in theory formation in exact science. We must develop on the one hand *concepts* (like 'the force due to gravity', 'the force due to charge'[13] or 'capacitance', 'resistance', 'impedance' . . .) and on the other, *rules for combination*; and what we assume about each constrains the other, for in the end the two must work together in a regular way.[14] When the concepts are instantiated in the arrangements covered by the rules, the rules must tell us what happens, where *regularity* is built into the demand for a rule: *whenever* the arrangement is thus-and-so, what happens is what the rule says should happen.

Developing concepts for which we can also get rules that will work properly in tandem with them is extremely difficult, though we have succeeded in a number of subject areas. In both physics and economics we have a variety of formal theories with special concepts and explicit rules that allow us to predict what regular behaviours should occur whenever the concepts are instantiated in the prescribed kinds of arrangements. And in physics, where we have been able to build clear samples of these arrangements, a number of our formal theories are well confirmed. Economics generally must rely on a more indirect form of testing, and the verdicts there are far less clear. At any rate, the success in various branches of physics in devising special concepts and laws that work in cases where the concepts clearly apply shows that there are at least some domains where the requirements we have been discussing are not impossible to fulfil.

A common metaphysical assumption about the completeness (or completability) of theory would go further and put an even more severe demand on our scientific concepts. The assumption was well expressed by John Stuart Mill:

> The universe, so far as known to us, is so constructed that whatever is true in any one case is true in all cases of a certain description: the only difficulty is to find what description.[15]

The sense of *completeness* I have in mind is this: a theory is complete with respect to a set of cases when it supplies for those cases the descriptions that Mill expects plus the principles that connect the descriptions.

[12] This is in sharp contrast with the method of representation just discussed in which factors with different capacities are combined in a single equation. Generally in these cases the value of the other relevant causes can be set to 'zero' to represent situations in which they do not operate.

[13] In my vocabulary these would be called 'the Coulomb capacity', 'the capacity for gravitational attraction' and so on.

[14] This point, I take it, is similar to that of Donald Davidson in Davidson 1995.

[15] Mill 1843, vol. 1, p. 337.

What about this additional demand? Should we accept it? I urge 'no'. The constraints imposed on concept formation in exact science by the demands to build at the same time a system of matching rules that will work together with the concepts in the right way are so severely confining that we have only satisfied them in a few formal theories in physics, and with great effort. And even in physics, we never have had a success, nor a near success, at completeness. It is only subject to the big *ceteris paribus* condition of the operation of an appropriate nomological machine that we can ever expect, 'that whatever is true in any one case is true in all cases'. We might well of course aim for completeness in any case where we have an empirically well-grounded research programme that offers promising ideas for how to achieve it. But in general we have no good empirical reason to think the world at large lends itself to description by complete theories.

This is why the idea of a nomological machine is so important. It is, after all, only a philosophical concept, like 'unconditional law' or 'complete theory' or 'universal determinism', a way of categorising and understanding what happens in the world. But it has the advantage over these that it adds less than they do to what we are given in our observations of how successful formal theories work; and it shows that we do not need to use these more metaphysically extensive concepts in order to make sense of either the successes of our exact sciences nor of the pockets of precise order that these sciences can describe. Where there is a nomological machine, there is law-like behaviour. But we need parts described by special concepts before we can build a nomological machine. The everyday concepts of *irritability* and *inaccuracy* will not do, it seems, nor the concept of acceleration in terms of rate of change of velocity with distance (dv/dx) rather than with time (dv/dt), which the Medievals struggled to make a science of. We also need a special arrangement: a bunch of resistors and capacitors collected together in a paper bag will not conduct an electric current. When we understand it like this, we are not inclined to think that exact science must be completable, at least in principle, in order to be possible at all.

There is one further central aspect of nomological machines that I have so far not discussed: *shielding*. Recall the irregularity in the orbit of Uranus from the point of view of the original model of the planetary machine. This reminds us that is not enough to insist that the machine have the right parts in the right arrangement; in addition there had better be nothing else happening that inhibits the machine from operating as prescribed. As we saw in chapter 1, even a very basic principle like $\mathbf{F} = m\boldsymbol{a}$ needs a shield before it can describe a regularity. We can have all the forces in all the right arrangements that license assignment of a particular 'total' force $\mathbf{F}$. But we cannot expect an acceleration $\boldsymbol{a} = \mathbf{F}/m$ to appear if the wind is blowing too hard. The need for shielding is characteristic of the ordinary machines we build in everyday life. The importance of the concept of shielding in understanding when regularities arise is a large part of the reason why it is so useful to think of the special arrangements that generate regularities as *machines*.

Models of certain kinds, I claim, function as blueprints for nomological machines. But we must not mistake this for the claim that the models we usually see in theories show us to build a nomological machine. The models are generally given at far too high a level of abstraction for that. Just think about the arrangements that must obtain in the model when we expect to do a vector addition of a number of forces represented there: the forces must all be 'exercised together'. And what does that mean? At least in the case of certain descriptive concepts, we get help from the bridge principles of the theory. But we generally get no advice at all in the case of arrangements. Even the bridge principles of course are little help in the actual building of a machine. Bridge principles tell us what the abstract concepts consist in more concretely. (For examples see chapter 8.) But what we are told is still too formal. We need to know about real materials and their properties, what the abstract concepts amount to *there*, before we can build anything. But telling us this is no part of theory. This is one of the reasons that I find the 'vending machine' view of scientific prediction, testing, or application that I discuss in chapter 8 so grotesque.

I began with models that lie entirely within a single exact science. These are the models that allow us to predict in a systematic and warranted way the kind of precise and regular behaviour that we see in the laboratory and in many of our carefully manufactured technological devices or even occasionally in nature as it comes. But not all regular behaviour is precise. Coarsely-tuned machines, like my old bicycle, can give us regular behaviour even though the descriptions under which the behaviour falls are in no way quantitatively precise. Nor are the predictions of models always warranted in this top-down way. In general we construct models with concepts from a variety of different disciplines, the arrangements in them do not fit any rules for composition we have anywhere and the regular behaviour depicted in the model does not follow rigorously from any theory we know. Yet these models too, whenever I look at one of them, seem well described as blueprints for nomological machines.

So here is my strong claim: look at any case where there is a regularity in the world (whether natural or constructed) that we judge to be highly reliable and which we feel that we understand – we can either explain the regularity or we believe it does not need explanation. What you will find, I predict, is that the explanation provides what is clearly reasonable to label as a *nomological machine*. And where there is no explanation needed you will still find a machine. Sometimes for instance the whole situation is treated as one simple machine (like the lever), where the shielding conditions and the idea of repeated operation are so transparent that they go unnoted. To the extent that this claim is borne out, to that extent we have powerful empirical evidence that you cannot get a regularity without a nomological machine. And if nomological machines are as rare as they seem to be, not much of what happens in nature is regular and orderly, as Mill supposed it to be. The world is after all deeply dappled.

4 Capacities: openness and invention

I argue against laws that are unconditional and unrestricted in scope. Laws need nomological machines to generate them, and hold only on condition that the machines run properly. But there are, as we saw in the last section, some very well understood machines, modelled within the various disciplinary boundaries of our exact sciences. I say our understanding of these depends on knowledge of capacities, not knowledge of laws. Is there much, after all, in the difference? I think so, because when we refuse to reconstruct our knowledge as knowledge of capacities, we deny much of what we know and we turn many of our best inventions into pure guesses. What is important about capacities is their open-endedness: what we know about them suggests strategies rather than underwriting conclusions, as a vending-machine view of science would require. To see the open-endedness it is useful to understand how capacities differ from dispositions.

Disposition terms, as they are usually understood, are tied one-to-one to law-like regularities. But capacities, as I use the term, are not restricted to any single kind of manifestation. Objects with a given capacity can behave very differently in different circumstances. Consider Coulomb's law, $F = -q_1q_2/4\pi\varepsilon_0r^2$, for two particles of charge q_1 and q_2 separated by a distance r. I will discuss this case in more detail in chapter 4. For here let us just consider what Coulomb's law tells us about the motions of the particle pair. It tells us absolutely nothing. Before any motion at all is fixed, the particles must be placed in a special kind of environment; just the kind of environment that I have described as a nomological machine. Without a specific environment, no motion at all is determined.

We may think that the *natural* behaviour for opposite charges is to move towards each other and for similar charges, to separate from each other. But it is important to keep in mind that this is not an effect *in abstracto*. That motion, like any other, depends on how the environment is structured. There is no one fact of the matter about what *occurs* when charges interact. With the right kind of structure we can get virtually any motion at all. We can even create environments in which the Coulomb *repulsion* between two negatively charged particles *causes them to move closer together*. Figure 3.1 gives an

Two electrons e_1 and e_2 are released from rest into a cylinder as in Figure 3.1b. The cylinder is open from one side only, and it is open to a unified magnetic field directed towards the negative z-axis. The initial distance between the two electrons is r_1. According to the laws of electromagnetism, the force between the two electrons is a repulsive force equal to

$$F = \frac{1}{4\pi\varepsilon_0}\frac{e_1 e_2}{r_1^2} = m_e a_a.$$

Whereas e_2 will be locked inside the cylinder, e_1 will enter the magnetic field **B** with a certain velocity v_1. The magnetic field on e_1 will move it in a circular motion (as in the figure) with a force equal to

$$F = ev_1 \times B.$$

This will take the electron e_1 into an insulated chamber attached to the cylinder. The dimensions of the cylinder and the chamber can be set so that the distance between the final position of e_1 and e_2 is less than r_1.

Figure 3.1a Source: example constructed by Towfic Shomar.

example, due to Towfic Shomar, from the LSE Modelling and Measurement in Physics and Economics Project.

For a different kind of example, let us turn to economics, to a study by Harold Hotelling[16] of Edgeworth's taxation paradox.[17] This is a case that I have worked on with Julian Reiss, also from the LSE Modelling and Measurement in Physics and Economics Project.[18] Taxes have the capacity to affect prices. How do we characterise the effects of this capacity? Think again about the capacity represented in Coulomb's law with respect to the motion of oppositely charged particles. We tend to characterise this capacity in the canonical terms I used above: opposite charges move towards each other; similar charges, away from each other. Similarly, *taxes increase prices*. The 'paradox' pointed out by Edgeworth is that this is not the only possibility. In the right situations taxes can decrease prices, and they can do so by following just the same principles of operation that 'normally' lead to price increase.

Hotelling produced a toy model of a simple economy that illustrates Edgeworth's paradox. The economy consists of many firms which compete in the production of the different commodities and many buyers whose sole source of utility derives from these goods. A version of the Hotelling economy with

16 Hotelling 1932.
17 *Cf.* Edgeworth 1925, section II.
18 See also Hands and Mirowski 1997 and my comments in Cartwright 1997c.

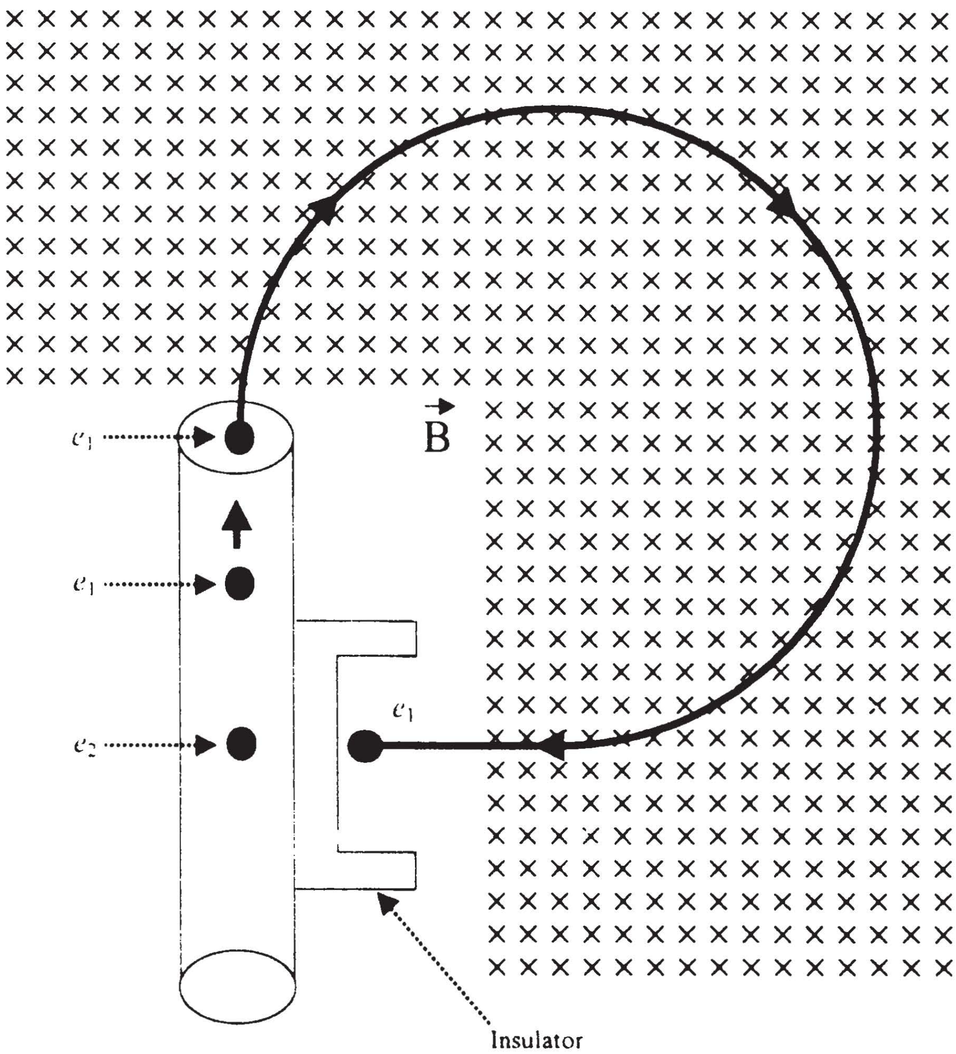

Figure 3.1b.

only two goods is described in Figure 3.2. If we consider a tax t levied on the first good only, what matters are two equations of the following forms:

$$dp_1 = tA/D$$

$$dp_2 = tB/D$$

A and B are functions, through the demand and supply equations, of partial derivatives with respect to each of the two quantities of functions of both quantities ($\delta f(q_1q_2)/\delta q_1, \ldots$). In the terminology I used in *Nature's Capacities and their Measurement,*[19] A/D represents *the strength of t's capacity to affect* dp_1, and B/D, *the strength of t's capacity to affect* dp_2. For Hotelling's two commodity economy, it can be shown that D is always positive. But it is possible to construct supply and demand functions whose parameters make

[19] Cartwright 1989.

The economy[1] consists of many firms who compete in the production of two different commodities and many buyers whose sole source of utility is from these two goods. The prices of the goods are p_1 and p_2, respectively, and the demand functions are given by the expressions:

$$q_i = F_i(p_1, p_2) \quad (i = 1, 2). \tag{1}$$

It is assumed that these equations are solvable, such that we have the inverse demand functions:

$$p_i = f_i(q_1, q_2) \quad (i = 1, 2). \tag{2}$$

For the producers the respective supply functions are given with the following expressions:

$$q_i = G_i(p_1, p_2) \quad (i = 1, 2) \tag{3}$$

$$p_i = g_i(q_1, q_2) \quad (i = 1, 2). \tag{4}$$

Now, let $h_i(q_1, q_2)$ be the excess of demand price over supply price. Thus we obtain,

$$h_i = f_i - g_i. \tag{5}$$

A differentiation with respect to a q_j will be denoted with a subscript j, so that,

$$f_{ij} \equiv \frac{\partial f_i}{\partial q_j}, \quad g_{ij} \equiv \frac{\partial g_i}{\partial q_j}, \quad h_{ij} \equiv \frac{\partial h_i}{\partial q_j}.$$

The last definition will be the determinant of the marginal excess price matrix:

$$D \equiv \frac{\partial(h_1, h_2)}{\partial(q_1, q_2)} = \begin{vmatrix} h_{11} & h_{12} \\ h_{21} & h_{22} \end{vmatrix} = h_{11}h_{22} - h_{21}h_{12}.$$

Let an asterisk * denote the equilibrium values for which supply and demand are equal. Then

$$h_i(q_1^*, q_2^*) = 0. \tag{6}$$

Now, a tax t_i per unit sold is imposed on the ith commodity, payable by the producers. Let $p_i + \mathrm{d}p_i$ and $q_i + \mathrm{d}q_i$ be the new prices and quantities. In equilibrium, the demand price must exceed the supply price by exactly t_i. Hence,

$$h_i(q_1^* + \mathrm{d}q_1, q_2^* + \mathrm{d}q_2) = t_i.no(7)$$

A Taylor expansion of the first order and the subtraction of equation (6) yield the following approximations for small t_i:

$$h_{i1}\mathrm{d}q_1 + h_{i2}\mathrm{d}q_2 = t_i. \tag{8}$$

Figure 3.2 Taxation under free competition in a Hotelling economy. Source: construction of this two-commodity economy is by Julian Reiss.

The solutions to these equations are:

$$
\begin{aligned}
dq_1 &= \frac{1}{D}\begin{vmatrix} t_1 & h_{12} \\ t_2 & h_{22} \end{vmatrix} = \frac{t_1 h_{22} - t_2 h_{12}}{h_{11}h_{22} - h_{21}h_{12}}, \\
dq_2 &= \frac{1}{D}\begin{vmatrix} h_{11} & t_1 \\ h_{21} & t_2 \end{vmatrix} = \frac{t_2 h_{11} - t_1 h_{21}}{h_{11}h_{22} - h_{21}h_{12}}.
\end{aligned} \tag{9}
$$

The interesting effect the tax has is on the prices. The price changes to buyers resulting from the taxes are:

$$
dp_i = -\frac{1}{D}\begin{vmatrix} 0 & f_{i1} & f_{i2} \\ t_1 & h_{11} & h_{12} \\ t_2 & h_{21} & h_{22} \end{vmatrix}. \tag{10}
$$

Or, more specifically, the h_{ij}s replaced by the corresponding $f_{ij} - g_{ij}$ and, following Hotelling's (1932) example, the tax levied upon good one only $(t_1 = t, t_2 = 0)$:

$$
\begin{aligned}
dp_1 &= \frac{t(f_{11}f_{22} - f_{11}g_{22} - f_{12}f_{21} + f_{12}g_{21})}{D} \\
dp_2 &= \frac{t(f_{22}g_{21} - f_{21}g_{22})}{D}.
\end{aligned} \tag{11}
$$

Edgeworth's taxation paradox arises when a tax levied upon a good decreases its price rather than increases it, as is normally expected. It can be shown that for two commodities $D > 0$.[2] With that result and the fact that $t > 0$ the conditions for Edgeworth taxation paradox to arise are:[3]

$$
\begin{aligned}
dp_1 < 0 &\Leftrightarrow f_{11}f_{22} - f_{12}f_{21} < f_{11}g_{22} - f_{12}g_{21}, \\
dp_2 < 0 &\Leftrightarrow f_{21}g_{22} - f_{22}g_{21} < 0.
\end{aligned} \tag{12}
$$

One can easily see the dual (or triple) capacity of the tax to increase or decrease (or leave unchanged) the prices, depending on the values of the other parameters. This dual capacity stems from the fact that the two goods interact both in consumption and production. For a single commodity equation (10) yields:

$$
dp = -\frac{1}{D}\begin{vmatrix} 0 & f_{11} \\ t & h_{11} \end{vmatrix} = \frac{tf_{11}}{h_{11}} > 0, \tag{13}
$$

since t is always positive and both f_{11} and h_{11} $(= f_{11} - g_{11})$ are negative.

[1] This example is a simplified version of the model of Hotelling (1932), Section 5.
[2] *Cf.* Hotelling (1932), p. 601.
[3] The slight differences between these conditions and Hotelling's original conditions (25) and (26) arise from the fact that Hotelling makes use of his integrability conditions that imply $h_{ij} = h_{ji}$.

Figure 3.2 cont.

A or B or both negative. So, depending on the specific structure of supply and demand in the Hotelling economy, taxes can increase prices, or decrease them, or the prices may even stay the same. Yet in all instances it is the same capacity at work, with the same functional form, derived from the same basic principles.

These two examples – of Coulomb's law and of the capacity of taxes to affect prices – illustrate why I talk of *capacities*, which give rise to highly varied behaviours, rather than of *dispositions*, which are usually tied to single manifestations. If we do wish to stick to the more traditional vocabulary of dispositions, then a capacity is what in *The Concept of Mind* Gilbert Ryle called a 'highly generic' or 'determinable' disposition as opposed to those that are 'highly specific' or 'determinate'. According to Ryle, verbs for reporting highly generic dispositions 'are apt to differ from the verbs with which we name the dispositions, while the episodic verbs corresponding to the highly specific dispositional verbs are apt to be the same. A baker can be described as baking now, but a grocer is not described as "grocing" now, but only as selling sugar now, or weighing tea now, or wrapping up butter now.'[20]

The point I want to stress is that capacities are not to be identified with any particular manifestations. They are rather like 'know', 'believe', 'aspire', 'clever' or 'humorous' in Ryle's account: 'They signify abilities, tendencies, propensities to do, not things of one unique kind, but things of lots of different kinds.'[21] This is why the idea of the nomological machine is so important when we think of using the knowledge we gather in our exact sciences to intervene in the world. Much of modern scientific theory is about capacities, capacities which can have endless manifestations of endless different varieties. That is the key to how scientific invention is possible. Similarly charged particles repel each other, opposite charges attract; what that can amount to in terms of the motions and locations of the particles is limited only by our imagination. Taxes affect prices, but what happens to the prices depends on the economics we build and how well we build them.

5 Do we really need capacities?

It is now time to defend explicitly my claim that we need claims about capacities to understand nomological machines and cannot make do with laws, in the necessary regular association sense of 'law'. I shall look at two prominent places where we can see why we need capacities instead of laws. The first is in the principles for building nomological machines, the second for describing

[20] Ryle 1949, p. 118.
[21] *Ibid.*, p. 119.

their running. It is important to the discussion to keep firmly in mind that there is more to the conventional sense of 'law' than regularity: the regularities are supposed to be ones between some especially favoured set of OK properties, say occurrent properties or ones we can measure. But these will never give us what we need.

Look first to the building of a nomological machine. In building the machine we compose causes to produce the targeted effect. Consider again Newton's principle of gravity and Coulomb's law. These two may work together, in tandem with Newton's second law of motion ($\mathbf{F} = m\mathbf{a}$), to explain the trajectory of a charged body. I say that Newton's and Coulomb's principles describe the capacities to be moved and to produce motion that a charged body has, in the first case the capacity it has on account of its gravitational mass and in the second, on account of its charge. How should we render Newton's principle, instead, as a claim about regular associations among purely occurrent or directly measurable properties?

The relevant vocabulary of occurrent or measurable properties in this case is the vocabulary of motions – positions, speeds, accelerations, directions and the like. But there is nothing in this vocabulary that we can say about what masses do to one another.[22] As we saw in section 4, when one mass attracts another, it is completely open what motion occurs. Depending on the circumstances in which they are situated, the second mass may sit still, it may move towards the first, it may even in the right circumstances move away. There is no one fact of the matter about what occurrent properties obtain when masses interact. But that does not mean that there is no one thing we can say. 'Masses attract each other.' That is what we say, that is what we test, in thousands of different ways; and that is what we use to understand the motions of objects in an endless variety of circumstances.

'Masses attract each other.' Perhaps this is what regularity theorists had in mind all along. But if so, they have given up the resistance to capacity talk altogether. What occurrent or directly measurable properties do two bodies have in common when they are both attracted to another body? None. Similarly two masses that are both busy attracting other bodies are crucially alike, but not in any way that can be described within the vocabulary of measurable or occurrent properties. Think about Gilbert Ryle's arguments in *The Concept of Mind*. When we use the term 'attract' in the consequent of a regularity claim, we do just what Ryle warns us against in the case of mental dispositions: we categorise together as one kind of episode all the results that happen

[22] The one case we have looked at where the basic principles could legitimately be thought of as describing what systems do using only occurrent-property language is in the simultaneous equations models of econometrics. The equations are supposed to involve only measurable quantities, and since each equation must be separately satisfied, the relations between measurable quantities that really occur are literally in accord with each of the principles.

when two masses interact, whatsoever these episodes look like. The *Concise Oxford English Dictionary*,[23] for instance, defines 'attract' when used 'of a magnet, gravity, etc.' as 'exert a pull on'. 'Attract' and 'pull' are like 'groce' for the activities of a grocer and 'solicit' for the activities of a solicitor. They are not in the usual philosopher's list of occurrent property terms. Rather, they mark the fact that the relevant capacity has been exercised. That is what is in common among all the cases when masses interact as Newton described.

Sometimes we conceal the widespread use in physics of terms like 'attract', terms that mark the exercise of a capacity, by a kind of equivocation. We switch back and forth between an occurrent sense of the term – a body has *attracted* a second when the second moves towards it – in which Newton's principle or Coulomb's is generally not borne out – and the sense marking the exercise of a capacity in which the principles do seem to be true (if not universally at least as widely as we have looked so far). 'Attract', like many verbs in both ordinary and technical language, comes with a natural effect attached, and with two senses. In the first sense the natural effect must occur if the verb is to be satisfied; in the second sense, it is enough for the system to exercise its capacity regardless of what results, i.e., for it *to try to produce the associated effect*.

The trying is essential, and sometimes verbs like these have it built right into their definition. To 'court', according to the *Concise Oxford Dictionary*,[24] is to 'try to win the affection or favour of (a person)'. These kinds of words are common in describing the facts of everyday life: to brake – to apply the brakes, or to succeed in slowing the vehicle; to anchor – to lower the anchor, or to succeed in securing the boat; to push, to pull, to resist, to retard, to damn, to lure, to beckon, to shove, to harden (as in steel), to light (as the fire). . . .; and especially for philosophers: to 'explain' is not only, in its first sense in the *Concise Oxford Dictionary*, to 'make . . . intelligible', but also, in its second, to 'say *by way of* explanation'.[25]

The technical language of physics shares this feature with our more ordinary language; indeed it shares much of the same vocabulary. *Attraction, repulsion, resistance, pressure, stress*, and so on: these are concepts that are essential to physics in explaining and predicting the quantities and qualities we can directly measure. Physics does not differ from ordinary language by needing only some special set of occurrent property terms or directly measurable quantities stripped of all connections with powers and dispositions. Rather, as I described in section 3, what is distinct about the exact sciences

[23] 8th edn, 1990.
[24] *Ibid.*
[25] *Ibid.*, italics added.

is that they deal with capacities that can be exercised not only more or less – push harder, or resist less – but with ones for which the strength of effort is quantifiable, and for which, in certain very special circumstances, the exact results of the effort may be predictable.

The second place where it is easy to see the need for capacity concepts is when we set the nomological machine running. This is a point I make much of at other places, so I will only summarise here. Consider the very simple case of two charged bodies separated by a distance r. To calculate their motions, we add vectorially the force written down in Coulomb's principle and the force written down in Newton's law of gravity; then we substitute the result into Newton's second law, $\mathbf{F} = m\boldsymbol{a}$. What then are we supposing? First, that there is nothing that inhibits either object from exerting both its Coulomb and its gravitational force on the other; second, no other forces are exerted on either body; and third, everything that happens to the two bodies that can affect their motions can be represented as a force. Notice that these caveats all have to do with capacities and their exercise. Nothing must inhibit either the charges or the gravitational masses from exercising their capacities. No further capacities studied in classical dynamics should be successfully exercised; and finally, the capacity of a force to move a body as recorded in Newton's second law must be exercised successfully, unimpeded and without interference.

Can we render these caveats without using the family of concepts involving capacities? Throughout these chapters I argue that we cannot. (In particular, I treat the first and second conditions in chapters 4 and 8; the third was discussed at length in chapter 1.) The idea that we can do so is part of the fundamentalist pretensions of physics: there is some vocabulary special to physics within which we can describe everything that matters to the motions of bodies. This view gains support, I take it, from a mistaken understanding about how deductivity works in physics. In theories like mechanics, electromagnetism and special relativity we have had considerable success in finding sets of occurrent property descriptions that have a kind of deductive closure: certain kinds of effects describable in that vocabulary occur reliably in circumstances where all the causes of these kinds of effects (and their arrangement) can be appropriately described within the designated vocabulary. But that does not cash out into regularity laws with the descriptions of the causes and their arrangements in the antecedents and the descriptions of the effects in the consequent. For we still need the shielding: nothing else must occur that interferes with the capacities of those causes in that arrangement to produce those effects.

The need for this kind of addition is often obscured by the plasticity of the language of physics. Sometimes terms in physics refer to genuinely measurable quantities that objects or systems might possess and sometimes the use of the very same terms requires truths about the operation of capacities for its satisfaction. This plasticity gives physics two different ways to finesse the problems I have been discussing – either narrow the range of the antecedent to include the *ceteris paribus* conditions right in it, or expand the range of the consequent to cover whatever occurs when the capacities in question operate.

We have seen lots of illustrations of the second device already, for instance with the introduction of words like 'attract' and 'repel' into Coulomb's law and the law of gravity. The first can be seen in the simple case of the law of the lever. 'Lever' can be defined in terms of occurrent properties, making no allusion to capacities and their exercise. So, we sometimes use 'lever' to mean *rigid rod*, where a rod is *rigid* just in case the distances between all the mass points that make it up remain constant through the motions of these mass points. But sometimes we use 'lever' only for rigid rods so placed that their capacity to exhibit the behaviour required in the law of the lever will operate unimpeded. In this sense (if physics is right about the capacities of rigid rods), then a lever is bound to satisfy the law of the lever.

So far I have argued that there are jobs we do – and indeed should do – with our scientific principles that cannot be done if we render them as laws instead of as descriptions of capacities. There is one answer to my plea for capacities that sidesteps these defences of capacities. The answer employs a kind of transcendental argument. It does not attempt to show how it is possible to do these jobs without capacities but rather tries to establish that it must be possible to do so. I borrow the form from arguments made by Bas van Fraassen and by Arthur Fine in debating more general questions of scientific realism.[26] The argument presupposes that we have available a pure data base, cleansed of capacities and their non-Humean relativities. The objection goes like this: 'You, Cartwright, will defend the design of given machine by talking about what impedes and what facilitates the expression of the capacities in question. I take it this is not idle faith but that in each case you will have reason for that judgement. These reasons must ultimately be based not in facts about capacities, which you cannot observe, but in facts about actual behaviour, which you can. Once you have told me these reasons, I should be able to avoid the digression through capacities and move directly to the same conclusions you draw with capacities. Talk of capacities may provide a convenient way to encode information about behaviours, but so long as we insist that scientific claims be grounded in what can be observed, this talk cannot contribute any new information.'

But what about this decontaminated data base? Where is it in our experience? It is a philosophical construction, a piece of metaphysics, a way to

[26] Van Fraassen 1980, Fine 1986.

interpret the world. Of course we cannot do without interpretation. But this construction is far more removed from our everyday experience of the world as we interact with it and describe it to others than are homely truths about triggering mechanisms, precipitating factors, impediments, and the like, which mark out the domain of capacities. Consider an adaptation of van Fraassen's objection to causes,[27] which is a version of essentially the same argument. The objection proceeds from the assumption that there is some defensible notion of a sensible property which is conceptually and logically distinct from any ideas connected with capacities. We are then confronted with a challenge to explain what difference capacities make. 'Imagine a world identical with our own in all occurrences of its sensible qualities throughout its history but lacking in facts about capacities. How would that world differ from our world?'

On one reading, this argument may be about sequences not of properties in the world but of our experiences about the world. These sequences are to remain the same, but we are to imagine that they are not caused in the usual way by what is going on in the world around us. This reading cannot be the one intended, though, since it does not cut in the right way, revealing special virtues for descriptions like 'is red' or 'is a jet-stream trail' in contrast with ones like 'has the power to relieve headaches' or 'attracts other charges, *qua* charged'.

I might further be invited to inspect my experiences and to notice that they are 'really' experiences of succession of colour patches, say, with capacities nowhere to be found. The philosophical dialogue along this line is well rehearsed; I merely point in the familiar directions. My experiences are of people and houses and pinchings and aspirins, all things which I understand, in large part, in terms of their capacities. I do not have any raw experience of a house as a patchwork of colours. Even with respect to colours, my experience is of properties like *red*, which brings to objects the capacity to look specific ways in specific circumstances. Sense data, or *the given*, are metaphysical constructs which, unlike capacities, play no role in testable scientific claims. Once there was a hope to mark out among experiences some raw pieces by using an epistemological yardstick: the 'real' experiences were the infallible ones. After a great deal of debate it is not clear whether this criterion even lets in claims about felt pains; but it surely does not distinguish claims like 'The stripes are red' from 'Your pinching makes my arm hurt' and 'Mama is irritable'.

The contemporary version of this argument tends, for these reasons, not to be in terms of sense experiences but in terms of sensible properties. But here there is a very simple reply. A world with all the same sensible properties as

[27] Van Fraassen 1980, ch. 5.

ours would already be a world with capacities. As I remarked above, redness is the property that, among other things, brings with it capacity to look just *this* way in normal circumstances, and to look systematically different when the circumstances are systematically varied.

Perhaps we are misled here by carrying over the conclusions of an earlier metaphysics, conclusions for which the premises have been discarded. These premisses involve the doctrine of impressions and ideas. In the immediately post-Cartesian philosophy of the British empiricists, sensible properties could be picked out because they looked like their impressions. Gaze at the first stripe on the American flag: redness is the property that looks like *that*. We do not have this copy theory; so we do not have properties that are identified like that. Correlatively, we can no longer make the same distinction separating powers and their properties as did these seventeenth-century empiricists. On their doctrine, the way things looked could get copied in the perceiver's impressions of them; but the various powers of the property could not. Since their ideas were copies of their impressions, necessarily their world, as imaged, had only inert properties.

But we do not have the copy theory of impressions, nor do we adopt this simple theory of concept formation. For us, there are properties, and all properties bring capacities with them. (Perhaps, following Sydney Shoemaker,[28] they are all just conglomerates of powers.) What they are is given not by how they look but by what they do. So, 'How does the Hume world differ from ours?' It would not differ. Any world with the same properties as ours would *ipso facto* have capacities in it, since what a property empowers an object to do is part of what it is to be that property. The answer is the same for a world with the same sensible properties. And what about a world the same with respect to all the look-of-things? That question may have made sense for Locke, Berkeley, and Hume; but without the copy theory of impressions and the related associationist theory of concept formation, nowadays it makes no sense.

6 Metaphysical aside: what makes capacity claims true?

The world is made up of facts, Wittgenstein taught us. As empiricists we should insist that it is these facts that make our scientific claims true. What

[28] Shoemaker 1984, ch. 10.

facts then are they that make our capacity claims true? Let me turn the question around and see what the traditional view has to say. What facts make law claims true in the necessary regular association sense of law? There are, I think, only two honest kinds of answer for an empiricist.

The first is that *regularities make law claims true*, real regularities, ones that actually occur. These are undeniably facts in the world, not for instance putative facts in some merely possible world. They are thus proper empiricist candidates for truth makers. But we know that this lets in both too little and too much. Start with too much. What about all the accidental regularities? There is an honest empiricist answer:[29] laws are those regularities that cover the widest range of occurrences in the most efficient way.[30] The objection that there are too few regularities was taken up by Bertrand Russell:[31] a good many of the claims we are most interested in, especially in contexts of forecasting and planning, are about situations that may never occur or only rarely get repeated. Russell claimed that physics solves this problem by using very abstract descriptions; at that level 'the same thing' does generally occur repeatedly. (So, for example, the trajectories of the planets and of cannon balls and of electrons in a cloud chamber are all supposed to instantiate $\mathbf{F} = m\mathbf{a}$.)

My objection is the same in both cases. I summarise the lessons argued for in various places throughout this book: there are no such regularities to begin with. Unless we take capacities robustly, Coulomb's and Newton's principle are ruled out immediately. Perhaps they are to get relegated to the status of calculational tools for getting 'real' regularities, like $\mathbf{F} = m\mathbf{a}$. But even this is not a true regularity without adding to the antecedent the caveat that the force *operates unimpeded*. Russell's proposal fares better; but, as I argued in section 5, only if we allow our abstract vocabulary to include terms like 'attract' and 'repel', terms that have implications about capacities and their operations built in. So regularity theorists cannot even get started unless they too take facts involving how capacities operate to be part of the constitution of the world.[32]

Alternatively, there are proposals[33] to take necessitation as one of the kinds of facts that make up the world. Then we can still be *empiricist* in the sense that we can stick to the demand that scientific claims be judged against facts

[29] *Cf.* Friedman, Earman, Kitcher.
[30] That is, what makes a law claim true are first, a regular association and second, facts about how much a given collection of regular associations covers versus how much another does.
[31] Russell 1912–13.
[32] Facts, for instance, of the form *X interfered with Y's capacity to do Z*.
[33] *Cf.* Maudlin 1997.

about the real world around us.[34] The drawback to this proposal from my point of view is not that it lets modal facts into the world but rather that it lets in the wrong kind of modal fact. The inversion of a population of atoms does not *necessitate* the emission of coherent radiation; it allows it. But it allows it in some very special way. After all, anything *can* cause anything else. In fact, it seems to me not implausible to think that, with the right kind of nomological machine, almost anything can *necessitate* anything else. That is, you give me a component with a special feature and a desired outcome, and I will design you a machine where the first is followed by the second with total reliability. Just consider, for example, Rube Goldberg machines or Paolozzi sculptures.

So, if anything can cause practically anything else, what is special about the claim that an inversion in a population of atoms allows (or can cause) coherent radiation? We can use the expression we often introduce in explaining our intuitions about laws of nature here: the inversion allows the coherent radiation *by virtue of the structure of the world*, or *by virtue of the way the world is made*. But what does that mean? To mark the distinction between the kind of accidental possibility, where anything can result in anything else, and this other more nomological sense of possibility, Max Weber labelled the latter '*objective* possibility'.[35] Weber's ideas seem to me very much worth pursuing in our contemporary attempts to understand scientific knowledge. But so far I still think that the best worked out account that suits our needs most closely is Aristotle's doctrines on *natures*, which I shall defend in the next chapter. Capacity claims, about charge, say, are made true by facts about what it is in the nature of an object to do by virtue of being charged. To take this stance of course is to make a radical departure from the usual empiricist view about what kinds of facts there are.

That returns me to the plea for the scientific attitude. Philosophical arguments for the usual empiricist view about what there is and what there is not are not very compelling to begin with. They surely will need to be given up if they land us with a world that makes meaningless much of what we do and say when we use our sciences most successfully. What makes capacity claims true are facts about capacities, where probably nature's grammar for capacities is much like our own – or at least as much like our own as any

[34] Perhaps I should say that this allows us to satisfy the *ontological* demands of empiricism. There are of course in addition in the empiricist canon also epistemological demands and demands about how meanings can be fixed. In my view, as I argue in different places here and in Cartwright 1989, all these kinds of demands are just as well met by claims about capacities as by claims about occurrent properties.

[35] *Cf.* Weber's *The Logic of Historical Explanation* in Runciman 1978, which is translated from Weber's 1951. In that essay, Weber attributes the concept of *objective possibility* to the German physiologist Johannes von Kries.

other claims about the structure of the world that we back-read from successful scientific formulations. What makes true, then, the claim, 'Inversion in a population of atoms has the capacity to produce coherent radiation'? In simple Tarski style, just that: the fact that inversion has the capacity to produce coherent radiation. And this fact, so far as our evidence warrants,[36] has as much openness about it with respect to determining occurrent properties, as does our own claim about the capacity.

7 Nomological machines and the limits of science

I have been defending the claim that facts about capacities and how they operate are as much a part of the world as pictured by the exact sciences as are facts about occurrent properties and measurable quantities. One may be inclined to query what all the fuss is about. Once we have forsaken the impressions-and-ideas theory of concept formation defended by Hume and all forms of sense-data theories as well, how are we to draw a distinction between facts about occurrent properties and ones about capacities in the first place?

I have no qualms about giving up the distinction. But in doing so we must not lose sight of one important feature of capacities that affects our doctrines about the limits of science. There is no fact of the matter about what a system can do just by virtue of having a given capacity. What it does depends on its setting, and the kinds of settings necessary for it to produce systematic and predictable results are very exceptional. I have argued here that it takes a nomological machine to get a regularity. But nomological machines have very special structures. They require the conditions to be just right for a system to exercise its capacities in a repeatable way, and the empirical indications suggest that these kinds of conditions are rare. No matter how much knowledge we might come to have about particular situations, predictability in the world as it comes is not the norm but the exception. So we should expect regularities to be few and far between. If we want situations to be predictable, we had better engineer them carefully.

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[36] Again I should add that I do not think there are any successful arguments that the evidence here is less good than for any of the more usual empiricist claims about what kinds of facts there are. Indeed, if my arguments are right, the evidence is much better since a reconstruction of scientific claims using capacity language will go very much farther in capturing our empirical successes than will a reconstruction that uses only the language of occurrent properties.

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